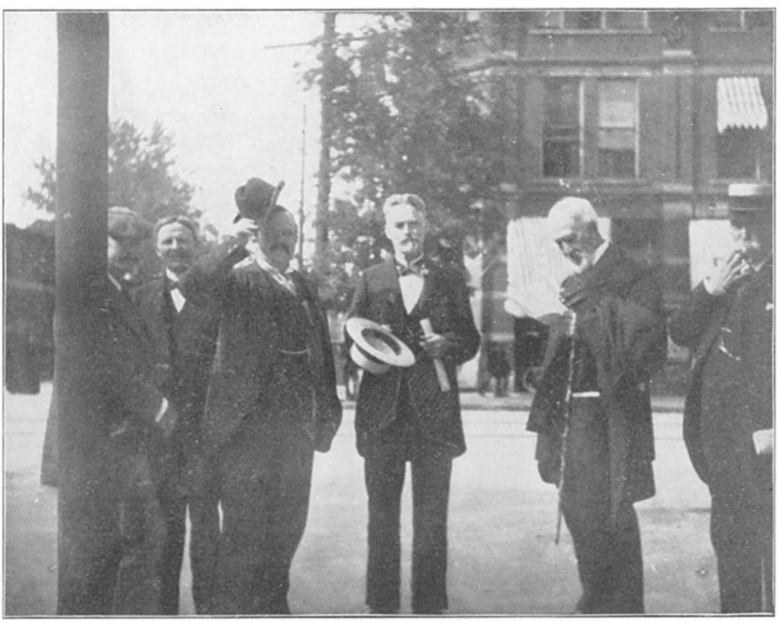

THE LAST PHOTOGRAPH OF PROFESSOR COPE.

Taken by Dr. F. C. Robinson at the Buffalo Meeting of the American Association for the Advancement of Science, 1896.

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EDWARD DRINKER COPE, NATURALIST—A CHAPTER IN THE HISTORY OF SCIENCE.[1]

BY THEODORE GILL.

I.

Bitter constraint, and sad occasion, dear,
Compels me to disturb your season due;
For Lycidas is dead, dead ere his time,
Our Lycidas, and hath not left his peer.

On the morning of the 13th of April, in a car on my way from a funeral in New York to Washington, a newspaper notice of the death, the day before, of my old friend, E. D. Cope, caught my eye. Shocked by the intelligence, I dropped the paper, and memory recalled various incidents of our long acquaintance.

The threnody of Milton[2] in commemoration of his friend Edward King, also rose to recollection, and the lines just quoted seemed to me to be peculiarly fitted for the great man just dead. He was, indeed, no longer young and had attained his prime,[3] but he had planned work for many years to come,

[1] Address by the retiring President of the American Association for the Advancement of Science at the Detroit Meeting, August 9th. Also printed in "Science," August 13, 1897, and in the "Scientific American Supplement," Aug. 14, 28, Sept. 4, 11, 1897.

[2] Milton, Poems, XVII.

[3] In the extract from Milton's poem, *time* has been substituted for *prime*, and *our* for *young*.

and had well advanced in the execution of some of it. He had truly died before his time and had left no peer; the greatest of the long line of American naturalists was prematurely snatched from science and from friends.

My acquaintance with Cope began in 1859. While looking through the part of the Proceedings of the Academy of Natural Sciences of Philadelphia for the month of April, in which my first paper published by the Academy had appeared, I found one by E. D. Cope "On the Primary Divisions of the Salamandridæ." It seems that the papers by Cope and myself had been passed on by the Committee on Publications on the very same day (April 26th), and appeared in print in juxtaposition. I had not previously heard of the new devotee of science, and read his article with as much interest as my own. A well-equipped man had evidently come upon the field and this was the first of the numerous articles that were destined to appear in an uninterrupted flow for nearly four decades. A few months afterwards I met the author in Philadelphia at the Academy. A young man, nineteen years old, about 5 feet 9 or 10 inches high, with head carried somewhat backwards and of rather robust frame, stood before me. He had an alert, energetic manner, a pronounced, positive voice, and appeared to be well able to take his part in any trouble. His knowledge was by no means confined to herpetology, but covered a wide range of science, and his preliminary education had been good. We afterwards met from time to time in Philadelphia and Washington, and found we had many sympathies in common and some differences.

In one of our first interviews we had quite an argument on the nature of the family group in zoology, resulting from criticisms I had made on the extended scope he had given to that category in the classification of the Salamanders. Another controversy, I remember, had reference to the vertebral theory of the skull. In an article on the venomous serpents, published in the Proceedings of the Academy for 1859, he had defined the group in terms involving the adoption of that theory, and I ventured to dissent from its reality. I had myself been much impressed with it in former days, and when 16 years old

had copied in colors an illustration of Owen's so-called archetype reproduced in Carpenter's Physiology. Subsequently, however, the fact that there was only an approximation to the realization of it in the most specialized of fishes and not at all among the lower or higher vertebrate, with other considerations, turned me from it, and I gave my reasons for dissent to Cope. Ultimately he admitted the force of the argument, and also abandoned the theory at one time so popular in England and America.

Our acquaintance, thus begun in 1859, continued uninterruptedly till death divided us. We rarely met, indeed, that we did not express difference of opinion respecting some subject, but the difference was never of a serious nature, and generally little more than sufficient to enliven intercourse.

II.[4]

The future naturalist was born in Philadelphia on the 28th of July, 1840, and the name Edward Drinker was given to him. He was the descendant of a prosperous line long established in Pennsylvania. His father, Alfred was a man of cultivated literary taste, and did much to train his son's mind in early youth. He had retired from active business and lived in luxurious ease near Germantown,[5] a suburb of Philadelphia. There he had formed an arboretum containing most of the American trees which would thrive in the climate of that region. Amidst such surroundings the youthful Cope grew up.

An active and intelligent interest in Nature became manifest at a very early age. When only about seven years old, during a sea voyage to Boston with his father, the boy is said to have kept a journal which he filled with drawings of "jelly fish, grampuses and other natural objects seen by the way."

[4] I am indebted to a brother in-law of Prof. Cope, Mr. Philip C. Garrett, for fuller information and rectifications of statements made in the original address, which I have utilized in this edition in the form of notes.

[5] According to Mr. Garrett, "in strict accuracy, his father either had not retired from active business or had never been in it, having been and remaining what is called an active partner of H. & A. Cope, though, it must be admitted, a rather inactive one at all times through very poor health. The home in which Edward was reared from early boyhood was not in Germantown, but about a mile east of it on the York road."

When eight and a half years old he made his first visit to the Museum of the Academy of Natural Sciences of his native city; this visit was on the "21st day of the 10th Month, 1848," as entered in his journal. He brought away careful drawings, measurements and descriptions of several larger birds, as well as of the skeleton of an Ichthyosaurus. His drawing of the fossil reptile bears the explanatory legend in Quaker style: "two of the sclerotic plates look at the eye—thee will see these in it."

At the age of ten he was taken upon a voyage to the West Indies.[6] What were the impressions he derived from that voyage we have not been told. But what has been communicated amply justified Professor Osborn in his declaration that "the principal impression he gave in boyhood was of incessant activity in mind and body, reaching in every direction for knowledge, and of great independence in character and action." His school[7] education was mostly carried on in the Westtown Academy, a Quaker institution about 23 miles west of Philadelphia. One of his instructors was Dr. Joseph Thomas, a well known literary worker of Philadelphia and future author of a "Universal Pronouncing Dictionary of Biography and Mythology" (1870), and said to have been an "excellent linguist." Under his guidance Cope obtained a passing knowledge of Latin and Greek. He appears to have had no instruction in any biological science and had no regular collegiate training. He did, however, enjoy the advantage of "a year's study (1858–9) of anatomy and clinical instruction at the University of Pennsylvania," in which the illustrious Leidy was professor of anatomy. But, in the words of his literary executor (Professor H. F. Osborn), "it is evident that he owed far more to paternal guidance in the direct study of nature and to his own impulses as a young investigator than to the five or six years of formal education which he received

[6] Osborn, in *Science*, N. S., V, 706.

[7] Mr. Garrett informs me that Cope's "education appears to have been received at home until 1851; then for two years at the Friends' Select School in Philadelphia; from 1853 to 1856 at West Town, and from 1856 to 1859 by private tuition, and then again at the Select School in Philadelphia.

at school. He was especially fond of map drawing and of geographical studies."

While a school boy he relieved his studies of the classics and the regular course in which boys of his age were drilled by excursions into the fields and woods. Reptile life espepecially interested him, and he sought salamanders, snakes and tortoises under rocks, stones, fallen trees and layers of leaves, as well as in the ponds and streams of his vicinage. The trophies of his excursions were identified from descriptions in the works in which they were treated, as well as by comparison with identified specimens in the museum of the Academy. He early and almost without guidance learned to use the library and collection of the Academy, although he did not become a member until he came of age in 1861.

Cope's first contribution to the Proceedings of the Academy appeared in the part covering April, and was "On the Primary Divisions of the Salamandridæ, with Descriptions of the New Species."[8] In this maiden paper he instituted important modifications of the systems previously adopted in the United States. He soon afterwards catalogued the serpents preserved in the museum of the Academy of Natural Sciences and likewise improved upon the systems previously in vogue. He continued with various papers, describing new species and giving synopses of brief monographs of sundry genera of lizards and anurous amphibians.

For five years his publication was confined almost exclusively to the reptiles and amphibians. (The continuity was only interrupted once in 1862, when he described a new shrew caught by himself in New Hampshire.)[9] Not until 1864 did he begin to extend his field. In that year he described various fishes and a supposed new whale, and gave his first contribution to paleontology in the description of the stegosaurian amphibian called *Amphibamus grandiceps.* But although his attention had become thus divided, he never lost his interest in

[8] Proc. Acad. Nat. Sci. Phila., 1859, pp. 122-128.

[9] An unentitled communication upon certain cyprinoid fish in Pennsylvania was published in the Proceedings of the Academy of Natural Sciences early in 1862 (Proc. 1861, p. 522-524); It is not included in the list of Cope's papers in the catalogue by the Royal Society.

herpetology and continued to the end of his life to devote much attention to that department. His studies extended to every branch of the subject, covering not only specific details and general taxonomy, but also the consideration of anatomical details, the modifications of different organs, geographical distribution, chronological sequence, genetic relations and physiological consequences. So numerous were his memoirs, so entirely did he cover the field of herpetology, and so marked an impression did he make on the science, that he was well entitled to apply to himself the boast of the Vergilian hero, "*Pars magna fui.*"

In his earliest essays he manifested the independence and critical spirit which were so characteristic of him later. One knowing all the circumstances of the case may be amused in coming across a passage expressed in the tones of a veteran published by him when 20 years old: "In proposing the name Zaocys * * * we are giving expression to an opinion *long held by us* as to the unnatural association of species in the so-called genus *Coryphodon* * * *. In it we find cylindrical terrestrial species united with compressed subarboricole species, upon a peculiarity whose value as an index of nature appears to us entirely imaginary. The very nature of the *coryphodontian* type of dentition, as distinguished from the *isodontian* and *syncranterian*, would lead us to infer its inconstancy;" and so on.[10] Bold as was the criticism of such herpetologists as Du-

[10] Proc. Acad. Nat. Sci., Phila., 1860, p. 563.

meril, Bibron and Günther, it was justified by the facts, and the young author's conclusions have received the endorsement of the best succeeding herpetologists, including even the latest author criticised.

In 1863 he paid a visit to Europe, partly for the benefit of his health which had suffered from overwork, and partly for the purpose of seeing the great museums of England, France, Holland, Austria and Prussia. Notwithstanding his ailments, he made good use of his time abroad and systematically examined the collections of reptiles in the chief centers of science. He did not even restrict his studies to herpetology, but extended them to various other subjects.

On his return from Europe in 1864, he was appointed professor of natural science in Haverford College, an institution chiefly supported by Quakers, but retained the position only three years. During this time, in 1865, he married Miss Annie, daughter of Mr. Andrew Pim, of Chester County, Pa.

In and after 1864, too, he enlarged the range of his studies and publications and also extended them to ichthyology, mammalogy and paleontology. He had always been interested in the philosophical aspects of science and early adopted the conception of descent with modifications to account for the variations of animals and the differentiation into species and higher groups, and in 1869 began to give expression to his peculiar views.

On the death of his father[11] he became heir to a considerable fortune. Part of this was invested in mines which for a short time gave promise of good returns, but, it is said, the majority of the stock was held by others, and owing to the incapacity of superintendents and the operations of the controlling stockholders, he losts his interests. While in the enjoyment of his fortune he spent large amounts in collections and personally conducted or sent out expeditions to various places. One of the most important was sent to South America. He filled a large house from cellar to topmost story with his collections and resided in an adjoining one.

In 1871 he conducted an expedition to Kansas and especially investigated the Cretaceous beds of that State and collected their fossils. In 1872 and 1873 he became connected with the U. S. Geological Survey, and for the fossils visited Wyoming in the former year and Colorado in the latter. In 1874 he joined the survey under the command of Lieut. Wheeler, of the Engineers, and explored New Mexico.

The collections made during these expeditions were large, and the unwearied industry and energy, as well as cares, of Cope were rewarded with many well-preserved fossils. These were described in many communications to the Academy of Natural Sciences and the American Philosophical Society, and later in large volumes published by the general government

[11] Cope's father died December 4, 1875.

as reports of the respective surveys with which he was connected.

The various investigations thus opened were continned through the succeeding years. His collections continued to grow in spite of reduced means. He refused even to sell portions for which he was offered liberal sums, and, at the cost of personal discomfort, held on to them and made his home, for much of the time, in the midst of them, having sold his residential house but kept his museum.

In 1878 he purchased the rights of the proprietors of the AMERICAN NATURALIST and removed it to Philadelphia. Professor Packard, one of the original proprietors, cooperated with him in the editing of it for some years, and he was also assisted by various eminent specialists. In this journal numerous articles of all kinds, including reviews and editorial comments, were published by him. His last words appeared in numbers issued after his death, the leading article in the number for June having been written shortly before his death; it treats of the remarkable mammals of South America, known as Toxodontia.

In 1886 he received an appointment to a chair in the University of Pennsylvania and became professor of geology and paleontology. Such a man naturally awakened the interest of apt pupils, and he was a facile and entertaining lecturer. From the stores of a rich memory he could improvise a discourse on almost any topic within the range of his varied studies. His views were so much in advance of those in any text-book that for his own convenience, no less than for the benefit of his pupils, he felt compelled to prepare a "Syllabus of lectures on geology and paleontology," but only "Part III, Paleontology of the Vertebrata," was published. It appeared in 1891, and is still a valuable epitome of the classification of the vertebrates, recent as well as fossil, giving in dichotomous tables the essential characters of all the groups above families and also the names of all the families. His own industry and investigations did much to render this antiquated in even six years, and a new edition of work became necessary. "Upon the Tuesday preceding his death he sent to the press an ela-

borate outline of his University lectures containing his latest ideas of the classification of the Vertebrata.[12]

The enormous mass of publications constantly flowing from his own pen might lead one unacquainted with the author to suppose that he was probably a recluse, but there were few men of his intellectuality who were less disposed to seclude themselves. He enjoyed and gave enjoyment to intellectual company and was a brilliant conversationalist. He was especially fond of academical meetings and was an unusually frequent attendant at the meetings of the American Association as well as of the National Academy of Sciences. His election to the Presidency of the American Association was highly esteemed by him and doubtless his address would have been a notable one.

In February (1897) Cope's health became seriously affected by a nephritic disorder, which, it is said, "might possibly have been remedied by a surgical operation," but to this he would not submit.[13] Notwithstanding failing health, he continued active almost to the last. Finally, the insidious disease invaded his entire system and he died on the 12th of April, in the room he had long used as a study, surrounded by the objects of his life-long attentions.

Such were the chief episodes of Cope's individual life; the facts known are few, and the record belongs rather to his family than to us. But Cope's real life was in his work, and to the consideration of that work we may now proceed. Let us adopt the order in which he took up the subjects of his investigations and successively look into his contributions to herpetology (III), ichthyology (IV), mammalogy (V) and paleontology (VI); we may then examine his philosophical views and especially those relating to evolution (VII); finally we may attempt to forecast the position he is destined to enjoy in the history of science (VIII). To know him as he was we must

[12] Osborn in *Science*, May 7, p. 705.

[13] According to Mr. Garrett, "as regards the disorder of which he died, it was cystic, not nephritic, the *post-mortem* showing little disease of the kidneys. The surgical operation he intended to undergo, but became too ill before it was accomplished."

recognize his weakness as well as his strength. He himself has wished this and has asked in the spirit of the Moor:

> Speak of me as I am; nothing extenuate,
> Nor set down aught in malice.

III.

The extent of Cope's contributions to herpetology have been referred to. Herpetology was his first love and continued to be the favorite branch of science to his life's end. His impress on it was, in some respects at least, greater than on any other of the sciences he cultivated, and doubtless the systems he introduced, with some modifications, will be the most lasting. He found herpetology an art; he left it a science: he found it a device mainly for the naming of specimens; he left it the expression of the coordination of all structural features. The reformations he effected in the classification of the anurous amphibians and the saurian reptiles were especially notable.

The anurans had been chiefly differentiated in groups on account of the most superficial characters. Such were the modes of fixation of the tongue or its absence, the development of disk-like expansions of the tips of the toes or simply attenuated toes, and the presence or absence of teeth in a jaw. Cope proceeded to investigate the group in an anatomical manner and reached entirely new conclusions. He found that important differences existed in the structure of the sternum, and especially in the connection of the lateral halves. In the common toads and tree toads of Europe and North America the so-called clavicle and coracoid of each side are "connected by a longitudinal arched cartilage which overlaps that of the opposite side," while in the common frogs the clavicles and coracoids of both sides are connected by a single median cartilage. The former type is now known as the arciferous and the latter as the firmisternal. Although Cope was the first to appreciate the significance of those characters, he did not at once fully realize their morphological value, the name Arcifera having been originally applied by him only to types of that group having teeth. Ultimately he did so, and his views have stood the test of time and the latest critical investigations.

He also found that the characters so revealed served to fix the places in the system of the groups in question. In their early stages the Firmisternials (or frogs and their relations) have the shoulder-girdle moveable, and thus resemble the Arcifers (toads, etc.), which have the opposite halves movable during their whole life time; thus it became evident that the latter are the lowest or most generalized forms, and the former more advanced and higher in the system. The development of teeth, which had been supposed by the earlier systematists to be of paramount value, and which Cope, following in their footsteps, had also originally unduly valued, has been found to be of quite subordinate importance.

The lizards were also in former times distributed into families and other groups on account of variations in superficial or external characters, such as the form of the tongue, the arrangement of the scales and the development of legs and feet. Cope dissected examples of all the types he could obtain and found that such superficial characters were often misleading, and he proceeded to arrange them with reference to the preponderance of all characters. The structure of the cranium especially was analyzed, and the variations and concordances in the development of various bones were tabulated. These characters were supplemented by others derived from the vertebræ, the shoulder girdle, the teeth, the tongue and the pholidosis. Familiarity with his subject enabled him almost instinctively to assess the relative values of the different characters, and he obtained fitting equations which resulted in a system which has received the approbation of the most competent judges to the present time.

The extent of Cope's influence on herpetology may be to some extent inferred from the catalogues of the richest collection of reptiles and amphibians in existence—the British Museum's. Descriptive catalogues of both the Anurans and Saurians have been published at different times. In the early catalogues are adopted the views current at the dates of publication—1845 for the lizards; 1858 for the batrachians. New editions were published many years later and the systems of Cope were adopted with slight modifications. In his catalogue

on the *Batrachia salientia* Mr. Boulenger, the author, remarked that it appeared "undeniable that the principles of classification laid down by Mr. Cope are more in accordance with the natural affinities of the genera of tailless Batrachians than those employed by other authors; this is amply proved by all we know of their geographical distribution, development and physiology."

In an article[14] published in advance of his catalogue of the lizards, Boulenger states that the old classifications are, "on the whole, as unnatural as can be" and that, "like Cope, whose lizard families I regard as the most natural hitherto proposed, I shall lay greater stress on osteological characters and on the structure of the tongue."

It was a long time, however, before Cope's views became popular. Even anatomists of repute refused to follow him. One[15] of them, for example, admitted that "skeletal characters are, indeed, most valuable ones in leading us to detect the deepest and truest affinities of vertebrates, but [he urged] these affinities once found, it is very desirable that zoological classification should not, if it can possibly be avoided, *repose* upon them only, but rather on more external and more readily ascertainable characters." He, therefore, ventured "to propose a classification derived from that of Dr. Günther."

Cope replied[16] by a fierce review of the work of Dr. Günther, and concluded with the utterance that such views "will only interfere with the progress of knowledge if sincerely held and believed."

But such views were evidently sincerely believed and they did retard the progress of science. An eminent Russian herpetologist objected to the use of anatomical characters. He especially protested against those employed by Boulenger after Cope to the grouping of the lizards, and Mr. Boulenger considered it incumbent on himself to defend the practice of using such characters;[17] he aptly replied that the use of "purely ex-

[14] Synopsis of the families of existing Lacertilia. Ann. and Mag. Nat. Hist. (5), XIV, 117.

[15] Mivart in Proc. Zool. Soc. London, 1869, p. 2[illegible]1.

[16] Cope in Am. Journ. Sci. (3), I, p. 203.

[17] Boulenger in Ann. and Mag. Nat. Hist. (5), X1X, 385.

ternal characters * * * does not meet the requirements of modern science," and that classifications are not made simply "for the convenience of beginners."

At last, however, the principles of classification adopted by Cope have become generally accepted, and doubtless this was in no small degree hastened by their application to all the amphibians and reptiles by Boulenger.

Cope's attention to the extinct reptiles was excited by the examination and consideration of a Carboniferous lizard-like amphibian which he was requested in 1865 to report upon. It was a new species which he named *Amphibamus grandiceps* and considered to be the type of a new order to which the name Xenorachia was applied, but which he subsequently referred to the new comprehensive order Stegocephali.

He sought for specimens of the extinct species with as much enthusiasm as he had for the recent. Extinct and living he considered together and light was mutually reflected from the two to guide him in the perfection of the entire system. In 1869 he gave expression to the results of his studies in a well illustrated "Synopsis of the Extinct Batrachia, Reptilia and Aves of North America." This was supplemented in 1874 by addenda and a "Catalogue of the air-breathing Vertebrata from the coal measures of Ohio."

A rich field was opened to him in 1877, when he received the first instalment of reptilian remains from Texas, which were at first considered to be of Triassic age, but subsequently determined to be Permian. Successive instalments of amphibian as well as reptilian skeletons enriched his collection, and his investigations revealed a new and wonderful fauna rich in species and often differing widely from any previously known. These were described in many articles. The results for the amphibians were summarized in 1884 in a memoir on the "Batrachia of the Permian period of North America."

The Permian amphibians were found to vary much in the composition of their backbones. Instead of having single centra arranged in a continuous row as in existing Vertebrates, they had distinct bones on which were devolved portions of the functions fulfilled by the centra of higher Vertebrates.

Some had "the vertebral bodies represented by three segments each, a basal intercentrum and two lateral pleurocentra;" these were named "Ganocephali" and "Rhachitomi." Some "differ remarkably from all other Vertebrata in having between the centra another set of vertebral bodies, so that each arch has two corresponding bodies;" these were called "Embolomeri."

In tracing the development of these bones, Cope came to the conclusion that they were only partially represented in higher or more specialized types; they did not become consolidated, but one or the other became reduced and finally lost or at least greatly atrophied. In the living amphibians the vertebral centra are homologous only with the intercentra, while, on the contrary, the centra of the reptiles, birds and mammals are represented by the pleurocentra of the Rhachitomes.

The studies of Cope on those classes which had earliest attracted his attention were more nearly completed than for any others. Many years ago he had contemplated the publication of monographs of the amphibians and reptiles of North America and happily he had at last finished his work.

In 1889 his monograph of the "Batrachia of North America" was given to the world as a Bulletin of the United States National Museum (No. 34). It forms a goodly volume of 525 pages illustrated by 81[18] plates and 120 figures inserted in the text. No large country has a more elaborate and scientific exposition of the class than is given in this volume. A synopsis is furnished of all the families and genera wherever found, and detailed descriptions are supplied for all the groups and species represented in the zoological realm of North America, 31 genera and 107 species are recognized, and of these Cope had first made known about a quarter, 7 of the genera and 27 of the species having been described by himself.

Shortly before his death, and during his last visit to Washington he delivered to the National Museum the report on all the reptiles of North America which he had been long preparing. This was prepared on the model of his "Batrachia of North America," but will, of course, be a much larger work,

[18] The last plate is numbered 86, but five were cancelled, 80, 81, 82, 84 and 85.

inasmuch as there are nearly three times as many reptiles as batrachians.[19] His last elaborate memoirs dealt with special anatomical features of the serpents and lizards, which he examined with the view of perfecting the system of those groups.

IV.

In 1864 Cope[20] became especially interested in the fresh-water fishes of the United States, and then as well as in succeeding years published enumerations and descriptions of many species. His first papers in 1864 and 1865 were "On a blind Silurid from Pennsylvania" and a "Partial catalogue of the cold-blooded Vertebrata of Michigan;" in 1868 he published "On the distribution of fresh-water fishes in the Allegheny region of southwestern Virginia," and in 1869 appeared a "Synopsis of the Cyprinidæ of Pennsylvania." In addition to these, various minor papers were published, and in some of them marine forms were considered.

When in Europe he had purchased a large collection of skeletons of fishes from all parts of the world prepared by Professor Joseph Hyrtl, of Vienna, one of the most skillful practical anatomists of the day. He had a number of other skeletons made to represent missing types. With these as a basis he proceeded to recast the classification of fishes. The first contribution to the subject was embodied in an introductory chapter of his "Contribution to the Ichthyology of the Lesser Antilles," published early in 1871.

The same chapter, with the same title, "Observations on the Systematic Relations of Fishes," but with some modifications and additions, was later published in the Proceedings of the American Association for the Advancement of Science for 1871. This was a notable paper and replete with original observations of value. It was not, however, up to the standard of his work on amphibians and reptiles. The subject, indeed, was too vast and only a superficial examination was made of

[19] Cope's monograph of the reptiles will not include the tortoises, those having been left to Dr. G. Baur to monograph.

[20] A short unentitled communication (before alluded to) was pulbished as early as 1863.

special parts. It was not a classification based on the examination of the entire structure, but rather an exposition of the development of a few particular characters, which more experience subsequently convinced him were of less value than he had supposed. Nevertheless, in some respects the proposed classification was much in advance of those previously adopted, and useful hints were given for the further improvement of the system.

Later Cope followed up this attempt at the reformation of the ichthyological system with several others especially treating of extinct types. One of them, "On the classification of the extinct fishes of the lower types," was published in the Proceedings of the American Association for 1877. The results of his studies were summarized, in 1889, in "A synopsis of the families of Vertebrata," and two years afterwards (1891) with modifications, in an article "On the non-actinopterygian Teleostomi." These results were very valuable, and attention was for the first time directed to the importance and morphological significance of the skeletal fin structures of the ancient fishes long confounded under the name of Ganoids. Instead of this single order (or subclass) of the old systematists, he named four superorders of the Teleostomi or true fishes, and recognized seven orders, including the old ganoids after eliminating the Lepidosteids and Amiids, which were referred to the Actinopterygians. Only two of the seven orders are represented by existing forms—one (*Cladistia*) by the bichirs of Africa, and the other (*Chondrostei*) by the sturgeons.

His work on the extinct fishes was incomparably better than any that had been done before in the United States. He far surpassed all his predecessors, not only by his knowledge of morphological details manifest in the extinct as well as living forms, but by his keen philosophical instinct and taxonomic tact. But this philosophical instinct was sometimes at fault, and occasionally he indulged in the wildest speculations, for which he has, not unjustly, been taken to task. Yet even his blunders were the result of the facility of his mind in seizing and adapting the latest utterances of science. One notorious case may be given. The great Russian embryologist Kowal-

evsky published a memoir sustaining the thesis that the Tunicates were members of the vertebrate phylum, and that the larval stage of most of the species had the homological equivalent of the backbone of the true vertebrates. Cope foresaw the morphological consequences of this view and sought the vertebrates nearest the Tunicates. He settled upon some strange forms of the Silurian and Devonian times known as Pteraspids and Cephalaspids. They were the earliest known of vertebrates and, therefore, likely to be the most primitive in structure. Most of them had a shell-like encasement, composed of bone-like plates. He happened to find illustrations of the living *Chelyosoma*, a true Tunicate, having a system of plate-like indurations of the integument, somewhat similar in appearance to those of some of the ancient fishes. It was assumed that this mere superficial similarity indicated genetic relationship. To those acquainted with the structure of *Chelyosoma* this approximation seemed strange indeed; its anatomy was known and the form is simply a well marked relation of the typical Ascidiids, but highly specialized by the development of integumentary plate-like horny indurations. Histologically and otherwise they were very different from the plates of the extinct armored vertebrates. Cope's guess was simply the result of the tendency to jump at conclusions which he was constantly obliged to curb, and unfortunately he rushed into print before he had time to think. He soon reconsidered the case with calmer mind, and abandoned his hypothesis. Few men were ever more willing to reconsider evidence and retrace false steps than was he.

In spite of errors of detail and somewhat hasty generalization the ichthyological labors of Cope were unusually valuable contributions to science, and the progress of ichthyology has been much accelerated, not only by these labors, but by the investigations they challenged.

V.

Cope's attention was early drawn to the mammals. His first published article (1863) was a description of a supposed new Shrew found in New Hampshire, and in 1865 he described

various cetaceans. In 1868 he began the collection and investigation of the fossil mammals of the western territory, and thenceforward devoted the larger share of his attention to the description and restoration of the numerous new species which he from time to time brought to light. The previous investigators of the extinct mammals of America had almost exclusively confined themselves to descriptions and illustrations of the crania and dentition, but a new era was introduced when Marsh and Cope sent out exploring expeditions or themselves collected. No parts of skeleton were neglected; all were collected. Gradually the numerous bones from different parts of the skeleton were identified, and finally many of the beasts of old were resurrected into skeletons almost as complete as those just divested of muscles.

The discoveries resulting from such thorough work quite modified or even overturned old conceptions. It became evident that there was a great contrast between the development of the mammals and that of the invertebrates, and even, though in a less degree, of fishes. It appeared that there was a much more rapid process of evolution for the mammals than for the lower classes. All the mammals of the oldest of the Tertiary periods were strange and very unlike those of recent times, and no descendants of even the same families lived to be the contemporaries of civilized man. The views of the founder of vertebrate paleontology were also to a considerable extent subverted. Cuvier taught that there was always a coordination between the various systems of the animal frame and that from the remains or impress of one part the approximate structure of the other parts could be inferred. He even pushed this doctrine to such an extreme that he overlooked some obvious counter facts. One such case is so remarkable because it originated with Cuvier and was endorsed by Huxley[21] that it is worthy of mention here, and Huxley's introduction to it and translation of it may be given. Huxley himself protests against the too literal application of Cuvier's law, and recalls Cuvier's own reserve:

[21] Huxley: "Introduction to the Classification of Animals," 1869, in first chapter "On Classification in General."

Cuvier, the more servile of whose imitators are fond of citing his mistaken doctrines as to the nature of the methods of paleontology against the conclusions of logic and of common sense, has put this so strongly that I cannot refrain from quoting his words.[22]

"But I doubt if any one would have divined, if untaught by observation, that all ruminants have the foot cleft, and that they alone have it. I doubt if any one would have divined that there are frontal horns only in this class; that those among them which have sharp canines for the most part lack horns.

However, since these relations are constant, they must have some sufficient cause; but since we are ignorant of it, we must make good the defect of the theory by means of observation. It enables us to establish empirical laws, which become almost as certain as rational laws, when they rest on sufficiently repeated observations; so that now, whoso sees merely the print of a cleft foot may conclude that the animal which left this impression ruminated, and this conclusion is as certain as any other in physics or morals. This footprint alone, then, yields to him who observes it, the form of the teeth, the form of the jaws, the form of the vertebræ, the form of all the bones of the legs, of the thighs, of the shoulders, and of the pelvis of the animal which has passed by. It is a surer mark than all those of Zadig."

The first perusal of these remarks would occasion surprise to some and immediately induce a second, more careful reading to ascertain whether they had not been misunderstood. Some men, with much less knowledge than either Cuvier or Huxley, may at once recall living exceptions to the positive statements as to the coordination of the "foot cleft" with the other characters specified. One of the most common of domesticated animals—the hog—would come up before the "mind's eye," if not the actual eye at the moment, to refute any such correlation as was claimed. Nevertheless, notwithstanding the fierce controversial literature centered on Huxley, no allusion appears to have been made to the lapsus. Yet every one will admit that the hog has the "foot cleft" as much as any ruminant, but the "form of the teeth" and the form of some vertebræ are quite different from those of the ruminants, and, of course, the multiple stomach and adaptation for rumination do not exist in the hog. That any one mammalogist should make such a slip is not very surprising, but that a second

[22] Ossemens fossiles, ed. 4me, tome, 1r, p. 184.

equally learned should follow in his steps is a singular psychological curiosity.

I need scarely add that the law of correlation applied by Cuvier to the structures of ruminants entirely fails in the case of many extinct mammals discovered since Cuvier's days. Zadig would have been completely nonplussed if he could have seen the imprint of an Agriochœrid, a Uintatherid or a Menodontid.

I have given this quotation for two reasons: first, to indicate how the increase of our knoweldge has revolutionized old conceptions; and second, to show how even the ablest of men may stumble.

Cope has been much criticised for the mistakes and false generalizations he made. Unquestionably he did make many. But error seems to be inseparable from investigation, and if he made more than the other great masters he covered more ground and did more work. He was also, it must be admitted, more hasty than some others in that he availed himself of the more frequent means of publication he enjoyed.

The great merit of Cope's work on mammals is that he always considered the old and new—the extinct and recent—forms together. He refused to be bound by consistency or by precedent, either set by himself or others. Fresh discoveries opened new vistas to him, and he modified his views from time to time and as often as he received new evidence.

He introduced many new families in the system and sought to improve the system by the comparison of all the elements of the skeleton. He came to the conclusion that the affinities of the ungulate quadrupeds were best expressed by the manner of articulation of the bones of the carpus and tarsus; he associated those having the "carpal and usually tarsal bones in linear series" in a great order which he called Taxeopoda, and contrasted them with the Proboscidea and typical Ungulata, which he named anew Diplarthra. In the Taxeopoda he gathered many extinct families and associated with them forms of the existing fauna known as the Hyracoidea, Daubentonioidea, Quadrumana and Anthropomorpha. I cannot altogether assent to this collocation inasmuch as I think the

common characteristics of the three groups last mentioned—especially the structure of the brain and the development of the posterior cornua of the ventricles as well as calcarine sulci—justify the old order Primates. Nevertheless an important character was first appreciated in the composition of the podial bones, and fresh insight was obtained into the relations of ancient types.

I can only name a few more of Cope's discoveries in this connection. One was the generalization of "trituberculy," or the original development of three tubercles to molar teeth, and that subsequent modifications of the corresponding teeth were based on this original plan. Another was the remarkable *Phenacodus* of the Eocene, which was considered to be nearly in a line of descent for the Ungulates as well as the series culminating in man and which led him to the conception of the taxeopodous group.

The past history and genealogy of the Camels and their relations were likewise elucidated. In the present epoch only two nearly related types exist separated by half the globe—the true camels of central and northern Asia and the llamas of the Peruvian Andes. Cope revealed numerous species from various Tertiary beds and showed that the type was originally richly developed in America.[23]

VI.

Paleontology, from more than one point of view, may be divided into Invertebrata and Vertebrate. The subjects of the former are generally to be found in an approximately complete condition so far as the exterior is concerned, and early attracted the attention of investigators, often little familiar with recent zoology, and received names. The subjects of the latter—especially the higher types, as mammals, birds and reptiles—are rarely found, except in a fragmentary condition. Special knowledge of osteology, even to its minutest details, is requisite to successfully deal with such remains. Consequently

[23] Prof. Osborn in a recent letter has justly remarked, that "in the mammals I hardly feel you do Cope sufficient justice, his work has been so potent." The exigencies of time and space alone prevented me from doing that justice, and I may remedy that defect later.

the fossil vertebrates of the United States were neglected and left to the few who had cultivated the requisite knowledge to deal with them.

Another reason existed for the tardy attention to Vertebrate paleontology, which continued till nearly the last quarter of our present century in the United States. No deposits containing many fossil vertebrate remains had become known in the east. Zoologists interested in the past and in the genealogy of existing forms lamented the poverty of the United States, which contrasted with the richness of some parts of Europe. It was even thought that there was no hope of finding here such trophies of the past as the beds of the Paris Basin or those of Grecian Pikermi had yielded to European paleontologists. But all this was to be changed. Rumor had long before hinted that numerous skeletal remains could be found in certain parts of the wild west, but the information was very vague. Enough was known, however, to induce Professor Marsh to visit certain deposits of which he had heard. In 1870 he explored an Eocene lake-basin in Wyoming, drained by the Green River, the main tributary of the Colorado, and therein found numerous bones, belonging to almost all parts of the skeleton, of some remarkable gigantic mammals which he called Dinocerata. The results of this exploration interested Cope in the highest degree. He visited the same region in 1872, and thenceforth his attention to the Vertebrate paleontology of the western States and Territories was never interrupted. An intense rivalry arose between Professor Marsh and himself which, in time, it must be confessed, became very bitter. Nevertheless, as in most quarrels respecting facts, investigations were provoked by mutual recriminations which resulted in a more speedy accumulation of data and a more critical examination of those data than would have been likely under less perturbed conditions. Most of those data relate to morphological and anatomical considerations, and therefore belong rather to mammalogy and herpetology than to geology.

The relations of the ancient forms to each other in point of time; to those of other lands, and to those whose remains

were imbedded in other rocks, had necessarily to be investigated. The earliest conclusions of Cope were brought together and published in 1879 in a memoir on "The Relations of the Horizons of Extinct Vertebrata of Europe and North America."[24] He attempted therein to synchronize, or rather, homotaxially correlate the various ancient faunas of North America and "West Europe" from the "Primordial" to the "Pliocene." Naturally the greater part of the memoir was devoted to the consideration of the Tertiary divisions; of these he admitted for the American faunas six primary divisions, and four of these were dichotomously subdivided. Of the primary divisions three were referred to the Eocene, one (White River) to the Oligocene, one (Loup Fork) to the Miocene, and one to the Pliocene. The exposition thus made represents views not very different from those now held, although, of course, modifications in details have since been necessary.

The evolution of the various animal, and especially mammalian types, were also continually the subject of Cope's researches, and he attempted to trace the passage from those of the most ancient periods to those of later ones.[25]

VII.

Cope was not satisfied with the study of morphological details or simple taxonomy. He aspired to know how animals came into existence; why they varied as they did, and what laws determined their being. His was an eminently philosophical mind, but at the same time with a decided tendency to metaphysical speculation. In one of his earliest papers he manifested this tendency and it persistod through life. It is with much hesitation that I venture to give an exposition of his most salient views, for I must confess I do not altogether like his philosophy and am able to subscribe to it only in part.

[24] Bull. U. S. Survey Terr., V, 33–54.

[25] I have been reminded by Prof. Osborn of Cope's "discovery of the Puerco—Cope's greatest geological achievement—which Prof. Marsh still fails to recognize; also the definition of the John Day and Deep river beds," Prof. Osborn adds, that "practically the whole fauna of the Wasatch is also Cope's." I recognized these facts, but, as in herpetology and ichthyology, was obliged to limit my address and to refrain from going into details.

I cannot but wish that one of his numerous disciples could have been chosen for this task. But I must not pass it by, for it is the most characteristic feature of Cope's work and the one he most esteemed.

Cope began his public scientific career, it will be remembered, in the same year in which Darwin's long studies had fructified into his "Origin of Species."

As was quite natural with his keen instincts, Cope early adopted the doctrine of transmutation of species and recognized the truth that all the animals of the present epoch are descendents from those of past times with modifications which separate them as species, and eventually as representatives of genera, of families and orders differing from the earlier ones as we retrace the the steps of Time farther and farther back. He was not, however, satisfied with Darwin's theory, and denied that natural selection was a sufficient factor for differentiation. He would not admit that animals were passive subjects and that the slight variations which were manifested in the progeny of species were sufficient to enable Nature to select from and to fit for future conditions. He contended that the volition and endeavors of an animal had much to do with future progeny as well as its own brief life. In short, he claimed that characters acquired by animals through their own efforts or forced on them by various external agencies or accidents might be transmitted to their offspring. He further, first in a chapter in his "Synopsis of the Cyprinidæ of Pennsylvania," outlined, and later, in "The Origin of Genera," he elaborated, a peculiar theory characterized mainly by what he called (with Professor Hyatt) "the law of *acceleration* and *retardation*" in development. Darwin complained that he could never understand this law, and Cope complained that Darwin had not stated his views correctly in an attempted abstract. I therefore give Cope's views, restated in his own language, summarizing them years afterwards. "The following doctrines," he says, were taught:"

First, that the development of new characters has been accomplished by an *acceleration* or *retardation* in the growth of the parts changed. This was demonstrated by reference to a class of facts, some of which were new, which gave ground for the establishment of the new doctrine.

Second, that of *exact parallelism* between the adult of one individual or set of individuals and a transitional stage of one or more other individuals. This doctrine is distinct from that of inexact parallelism which had already been stated by von Baer. And that this law expresses the origin of genera and higher groups, because,

Third, they can only be distinguished *by single characters* when all their representatives come to be known.

Fourth, that genera and various other groups have descended, not from a single generalized genus, etc., of the same group, but from corresponding genera of one or more other groups. This was called the doctrine of *homologous groups.*

Fifth, the doctrine that these homologous groups belong to different geological periods, and,

Sixth, to different geographical areas, which, therefore, in some instances, are,

Seventh, related to each other in a successional way like the epochs of geological time.

Of these doctrines it may be observed that the first and second are now the common property of evolutionists, and are recognized everywhere as matter of fact. The names which I selected to express them have, however, only come into partial use. The author believes that, although the doctrine was vaguely shadowed out in the minds of students prior to the publication of this essay, it had not previously been clearly expressed, nor been reduced to a demonstration. Of the truth of the doctrine the author is more than ever convinced, and he believes that paleontological discovery has demonstrated it in many instances, and that other demonstratious will follow. The fourth proposition (that of homologous groups) is now held as a hypothesis explaining the phylogeny of various groups of animals. For the descent of one homologous group from another, the term *polyphyletic* has been coined. It remains to be seen whether the doctrine is of universal application or not. That homologous groups belong to different geological horizons, as stated under the fifth head, has been frequently demonstrated since the publication of the essay. That the sixth proposition is true in a certain number of cases is well known, and it follows that the seventh proposition is also true in those cases. The latter hypothesss, which was originally advanced by Professor Agassiz, is, however, only partially true, and the advance of paleontological study has not demonstrated that it has had a very wide application in geological time.

A proposition which was made prominent in this essay was that the prevalence of non-adaptive characters in animals proves the inadequacy of hypotheses which ascribe the survival of types to their superior adaption to their environment. Numerous facts of this kind undoubtedly in-

dicate little or no activity of a selective agency in nature, and do point to the existence of an especial developmental force acting by a direct influence on growth. The action on this force is the acceleration and retardation appealed to in this paper. The force itself was not distinguished until the publication of the essay entitled "The Method of Creation" [1871], where it was named growth-force or bathmism. The energetic action of this force accounts for the origin of characters, whether adaptive or non-adaptive, the former differing from the latter in an intelligent direction, which adapts them to the environment. The numerous adaptive characters of animals had by that time engaged the attention of the author, and he found that they are even more numerous than the non-adaptive. Some of the latter were accounted for on the theory of the "complementary location of growth-force.

We can only consider the "law of acceleration and retardation." Again it behooves us to seek his own definition:

a. The succession of construction of parts of a complex was originally a succession of identical repetitions; and grade influence merely determined the number and location of such repetitions.

b. Acceleration signifies addition to the number and location of such repetitions during the period preceding maturity, as compared with the preceding generation, and *retardation* signifies a reduction of the number of such repetitions during the same time.[26]

His meaning may best be inferred from his application to mankind. This was done in the following terms in 1872:[27]

Let an application be made to the origin of the human species. It is scarcely necessary to point out at the start the fact, universally admitted by anatomists, that man and monkeys belong to the same order of Mammalia, and differ in those minor characters, generally used to define a "family" in zoology.

Now, these differences are as follows: In man we have the large head with prominent forehead and short jaws; short canine teeth without interruption behind (above); short arms and thumb of hand not opposable. In monkeys we have the reverse of all these characters. But what do we see in young monkeys? A head and brain as large, relatively, as in many men, with jaws not more prominent than in some races; the arms not longer than in the long-armed races of men, that is, a little beyond half way along the femur. * * * At this age of the individual the distinctive characters are therefore those of *homo*, with the exception of the opposable thumb of the hind foot, and the longer canine tooth. * * *

[26] Proc. Am. Phil. Soc., 1871; Origin of the Fittest, p. 182.

[27] Penn. Monthly Mag., 1872; Origin of the Fittest, p. 11, 1887.

Now, in the light of various cases observed, where members of the same species or brood are found at adult age to differ in the number of immature characters they possess, we may conclude that man originated in the following way: that is, by a delay or retardation of growth of the body and fore limbs as compared with the head; retardation of the jaws compared with the brain case, and retardation in the protrusion of the canine teeth.

There is a good reason for thinking that fallacy is involved in this argument, and that quite a different interpretation should be put on the evolution of charasters in question. It is not the fore limbs that are retarded in man, but the hind limbs have become enlarged (compare the adult and the infant). There is not retardation, of the jaws, but a special teleological adaptation. Man has, for the most part, at least, discontinued the use of his teeth for war-fare, and, as a result of diminished use, the canines have become reduced and the diastemata of the dental series obliterated. The brain has grown after birth and become enlarged, and, as a consequence, the brain case has extended forward—the reverse of what occurs in the apes. Concomitantly with the diminished use of the teeth and jaws, the masseter and temporal muscles have become reduced, and the sagittal and lambdoidal ridges have consequently become atrophied. The ecarinate rounded voluminous calvarium is the result.

It has been claimed that the young of higher species "are constantly accelerating their development." In man, however, development is retarded, inasmuch as infancy and juvenility are prolonged far beyond the periods observed in our simian relatives.

Such examples as this give cause to believe that the "law of acceleration and retardation" has been at least unduly extended. Acceleration and retardation are, however, to a large extent, terms which express facts of evolution; whether the word law is applicable may depend on the meaning one gives the word.

The transmission of acquired characters was one of the accepted and most cherished dogmas of Cope, and the belief in transmissibility of such characters is an essential of the ereed

of so many who have become his followers in America that a special school came into existence known as the Neo-Lamarckian and also as the American School. My own prejudices have inclined me to that school. Nevertheless, when I have divested myself of such prejudices as well as I could, I have been compelled to admit that the evidence of the heredity of acquired characters was rather weak. There was, indeed, evidence for, as well as against, but that against the doctrine of the transmissibility of acquired characters seems to be the most weighty.

It is to be understood that the acquired characters considered in this connection are such as have been developed during post-natal life as a result of endeavors of the animal or of the influence of external agencies. The evidence presented has been mostly in support of the contention that the characters acquired have been directly inherited by offspring, and consequently the transition from the form not possessing the character to one having it is rapid. The evidence adduced has not been conclusive, to say the least. There is, apparently, a germ of truth in the proposition that acquired characters are transmitted, but in a modified sense, and the case has been weakened rather than strengthened by the evidence offered.

The evidence for inheritance of acquired characters was frequently given by Cope, and in his last published work—"The Primary Factors of Organic Evolution"—he marshalled the testimonies of many witnesses with his accustomed skill. He evoked "evidence from embryology," "evidence from paleontology," "evidence from breeding;" he considered the "characters due to nutrition," "characters due to exercise of function," "characters due to disease," "characters due to mutilation and injuries" and "characters due to regional influence;" he inquired into "the conditions of inheritance," and he fought against the "objections to the doctrine of inheritance of acquired characters." I have gone over all this evidence and yet I have not been convinced that the contention has been sustained that character acquired during the external life of an animal are transmitted. Many cases are alleged to sustain the "inheritance of characters due to mutilation and injur-

ies." Some of these may be considered as mere coincidences; others provoke skepticism for one reason or other. To discuss them would be out of place here. But at least we may meet evidence with counter-evidence.

On the one hand, all the data and experiments recapitulated in the cases enumerated concern only two, or, at most, very few, generations of the animals in question, and were within the compass of a single man's life-time.

On the other hand, we have data and observations of the most reliable nature, and of an extraordinary compass. These have resulted not from experiments for the determination of a specific question, but from observances of a religious character. They were really in the nature of surgical operations, but for our purpose may be looked upon as experiments, and have the value of contrived experiments. In no other field has such a series of disinterested experiments been available. They were conducted on countless millions of mankind and for thousands of years. The subjects experimented upon were kept isolated from others alike by their own prejudices and the prejudices of their neighbors. Circumcision is the term applied to the experiments in question.

For about 4,000 years circumcision has been practiced on a gigantic scale. Every male child among the Jews was operated upon, not only in Palestine, but wherever representatives of the race had wandered and adhered to their religion; religion itself was involved in the operation and it was regarded as a holy rite; the most scrupulous attention was paid to details. The operation was performed eight days after birth, and consequently there could be no functional activity of the tissues concerned. But after 4,000 years the new-born boys of the race come into the world with the special integument developed as much as in those of other races. Even the principle of atrophy through disuse has not become manifest in the case.

Other evidence, it seems to me, is the result of confounding the potentiality of a function with its manifestation. I allude to one set of examples on account of the interest of the cases, and I do so with the deference due to the eminence and abil-

ity of the gentleman who has furnished the evidence. That evidence has been collected under the head of "inheritance of characters due to the exercise of function." The evolution of the American trotting-horse was considered. It was recorded that "by 1810 the taste for trotting as a sport had * * * increased here, and in 1818 it became a recognized sport under specific rules." * * * "At the end of 1824, six years after the first accepted three-minute record, the record had fallen to 2:34." * * * "By 1848 the record was lowered to 2:29½; the next decade lowered the record five seconds." Finally, at the close of 1895, the record had been further lowered to 2:03¾. * * * It is deduced from these premises that "there is nothing whatever in the actual phenomena observed anywhere along the line of this development of speed that would lead us to even suspect that the changes due to exercise of function had *not* been a factor in the evolution." But to me it seems that there is no evidence to show that the speed attained was other than would have resulted from taking the same animals untrained and then speeding the last. The speed is, of course, simply the expression of functional adaption, and the horses were selected merely because, by their manifestation, they showed that they had the co-ordination of structural and psychological characters needed for the manifestation of the function. The manifestation guided the breeder to the selection of the animals. The successful animals were the pick of thousands unknown to fame.

But there is much in the history of the development of animals that seems to lead to the belief that eventually modifications may be due in part to acts of representatives of the phylum to which they belong. It is difficult to believe that some structural features are simply the result of natural selection operating on chance variations. An application of the doctrine of chances to some such cases appears to be adverse to the conception that they represent the influence of natural selection unaided.

A feature characteristic of most cave animals of widely diverse groups and classes is the atrophy of the eyes, and it seems to be most logical to attribute this to disuse of those organs in

remote progenitors, and to assume that the atrophy may have resulted from a failure of nourishment by the nutrient fluid of the organs on account of the loss of functional activity rather than to selection by nature of forms with successively diminishing eyes. The presence of eyes in most cases certainly would scarcely be an element of disadvantage to animals, and it may be allowable to invoke some other agency than chance selection. We may be justified in postulating that the continuous disuse of the organs would in time react on the nutrition of the parts affected, and finally atrophy or disappearauce would result. Like explanation would be applicable to the innumerable cases of atrophy of parts known to the naturalist.

But if cessation of nutrition culminates in final atrophy, increased nutrition of parts may result in hypertrophy and increased nutrition may be the concomitant of increased activity of parts. The exercise of such parts continued for many generations may react on the organization and the progeny at length be affected thereby. Of such cases Cope adduced many examples. The feet of the horse line furnish illustrations. The existing horse has the median toes and hoofs greatly hypertrophied and the lateral ones atrophied, but the remote ancestors had feet of nearly the same general pattern as the rhinoceroses and tapirs. Atrophy of the lateral digits has progressed inversely to hypertrophy of the middle ones. An analogous line of development culminating in feet superficially much like those of the horse was followed by another quite remote family of hoofed mammals, the Prototheriids of South America.

The idea of acceleration and retardation was associated by Cope with the idea that the course of evolution was determined from the beginning of things, and that life, to use his own words, is "*energy directed by sensibility or by a mechanism which has originated under the direction of sensibility.*" He maintained that "consciousness as well as life preceded organism," and he called this conception "the hypothesis of archæsthetism." This idea I refer to especially because it was broached in his vice-presidential address, delivered at the meeting of the American

Association for the Advancement of Science, in Philadelphia, in 1884.[28]

I am, myself, unable to comprehend consciousness except as a product or result of organization, and those who wish to learn more about Cope's views respecting the question must refer to one of his many papers.

Whatever may be thought of Cope's philosophical views, his presentation of them is always interesting, and some of them are illustrated with a wealth of facts that renders his communications valuable as repertories of well digested information. His first special paper, on "The Origin of Genera," published as early as 1868, is especially noteworthy for the mass of morphological data contained in it, and for the apt manner in which they are tabulated.

VIII.

I venture to conclude with some reflections on the rank that may be assigned to Cope in the world of science.

Among those that have cultivated the same branches of science that he did—the study of the recent as well as the extinct Vertebrates—three naturalists have acquired unusual celebrity. Those are Cuvier, Owen and Huxley.

Cuvier excelled all of his time in the extent of his knowledge of the anatomical structure of animals and appreciation of morphological details, and first systematically applied them to and combined them with the remains of extinct Vertebrates, especially the mammals and reptiles. He was the real founder of Vertebrate paleontology.

Owen, a disciple of Cuvier, followed in his footsteps, and, with not unequal skill in reconstruction and with command of ampler materials, built largely on the structure that Cuvier had begun.

Huxley covered as wide a field as Cuvier and Owen, and likewise combined knowledge of the details of structure of the recent forms with acquaintance with the ancient ones. His actual investigations were, however, less in amount than those of either his predecessors. He excelled in logical and forcible presentation of facts.

[28] Origin of Fittest, p. 425.

Cope covered a field as extensive as any of the three. His knowledge of structural details of all the classes of Vertebrates was probably more symmetrical than that of any of those with whom he is compared; his command of material was greater than that of any of the others; his industry was equal to Owen's; in the clearness of his conceptions he was equalled by Huxley alone; in the skill with which he weighed discovered facts, in the aptness of his presentation of those facts, and in the lucid methods by which the labor of the student was saved and the conception of the numerous propositions facilitated, he was unequalled. His logical ability may have been less than than that of Huxley and possibly of Cuvier. He has been much blamed on account of the constant changes of his views and because he was inconsistent. Unquestionably he did change his views very often. Doubtless some of those changes were necessitated by too great haste in formulation and too great rashness in publication. The freedom to change which he exercised, and which was exercised too little by at least one of his predecessors, was an offset to his rashness. He exercised a proper scientific spirit in refusing to be always consistent at the expense of truth.

His reputation at present is much inferior, at least among the people at large, to those of the men with whom he has been compared. Immediate reputation depends on various circumstances, some of which are quite adventitious, and it is often long before men find their true levels. It is scarcely premature to prophesy that Cope's reputation will grow and that in the future history of science his place will be at least as large as that of any of his predecessors.